arguments and illustrations, as well as the criticism upon them, are all beside the point, for they all dealt with contacts between "bodies," not between a body and an "ether" in which it was moving. We must therefore begin *de novo*, and we must start this time with some definition or explanation of what he means respectively by "the bodies" and "the ether" which surrounds them.

But the point I particularly wished to discuss was his view of the "identity" of energy. I do not think any such identity can be recognised, at any rate if we grant Prof. Lodge's own hypothesis that energy on being transferred from one body to another is always transformed from Kinetic to Potential energy, or *vice versâ*, for I maintain that potential energy, as such, belongs to a system of bodies not to any particular one of them, and so has no local habitation even though it has a name.

The law of the conservation of energy is usually expressed by the formula—

Kinetic + Potential Energy = Constant.

But if this is to be a physical law, and not a mere truism, its terms must be defined in such a way that it is not a mere formal consequence of their definitions. As to Kinetic energy, everybody is practically agreed in defining it as $\Sigma\frac{1}{2}mv^2$, or, which is the same thing, as $\Sigma\int mvdv$. But if we define potential energy, as Prof. Lodge would apparently have us do, as $\Sigma\int -\mathrm{F}ds$, the formula does not assert a physical fact—at least no new one—but is merely an identity. The equation of energy in this form would, indeed, be quite useless, for we should have to know the previous path of each particle in order to evaluate $\int \mathrm{F}ds$. And so we find that in the equation of energy, as used by mathematicians, the "Potential Energy" has nothing to do with any paths the particles may have described, but is a mere function of their present co-ordinates.

The truth is that the physical fact implied in the law of conservation is not that the *energy in general* is conserved throughout all changes in the system, but merely that the *kinetic* energy is always the same whenever the system returns to the same configuration; that term being held, if necessary, to include, not only geometical form, but such conditions as temperature, chemical or electrical state, &c.

The law of energy is then better stated thus: "In any independent system of bodies—

Kinetic energy + A function of the configuration of the system = Constant."

And we may, if we like, call this function "Potential energy," since it diminishes as the kinetic energy increases; but we have no right to assume *à priori* that it is the same sort of thing as kinetic energy. It is true that in some cases what used to be called potential energy is now regarded as in great part kinetic, but this can only be done if at the same time we change our conception of the "configuration" of the system. If we regard the energy stored in a reservoir of compressed air as kinetic instead of potential, we must include the average positions of its particles in our statement of the configuration of the system.

But the important conclusion to be drawn from this is that potential energy (*quâ* potential) does not belong to a single particle but to the system as a whole, or at least it can only be allocated to such portions of the system as may by themselves be regarded as independent systems. If ever all energy were explained to be kinetic energy, and if we could then explain how it comes to be transferred from one body to another, we might be able to trace the biography of a piece of energy as we might that of an atom of matter. But even if "potential energy" may thus be regarded as only a name used to veil our present ignorance of what has happened to the kinetic energy, it is still illogical to talk of the "identity" of energy till this veil has been removed. And I cannot see that anything Prof. Lodge has said helps us in the smallest degree to remove it.

EDWARD T. DIXON.

Trinity College, Cambridge, May 27.

On the Velocity of Propagation of Gravitation Effects.

IF, according to the accepted kinetic theory of gases, the velocities of molecules "vary between zero and infinity" (Maxwell): it must certainly result that frequently enormous velocities are accidentally attained by even gross molecules, and this produces no perceptible disturbance measured by us. It would be admittedly almost puerile to ask how high a velocity might normally be possessed by a large number of particles of matter (as an *à priori* question, that is), provided the particles be perfectly elastic, so that there is no jar at their encounters, but the movement goes on with perfect smoothness, so that its existence may escape detection by the senses. Moreover there is no resistance in space to free motion of material particles.

In regard to the effects of gravity then, the practical question for us (in regard to their elucidation) becomes, What is the velocity demanded for the transmission of gravity? This velocity, whatever it be (if very great, but finite), may then reasonably be considered to exist in matter in some form, or to be possessed by it. To assert *à priori* that the existence (say among particles of matter) of a velocity even many times that of light, is unlikely, or to view this with incredulity as an abstract fact apart from its possible utility—would seem to partake somewhat of the nature of a prejudice, due possibly to absence of adequate reflection.

A high normal velocity has the undoubted mechanical advantage of being able to produce a given dynamical effect by means of very small particles, *i.e.*, without demanding for such effect any large collective mass or the employment of a great quantity of material. Smallness in size moreover allows the particles to possess a very long mean path: and they have the advantage of occupying, *in toto*, very little room (although they may be relatively numerous).

Without going into the question of the *modus operandi* of such effects as explosions of gases, dynamite, &c., it at least appears manifest that by the rejection of "action at a distance," a store of motion of a very high intensity in the matter of space would be consistent with, or would be demanded in order to give some rational account of sudden developments or transferences of motion. It may appear questionable whether a normal velocity of matter in space only equal to that of light, would be sufficient to account for the explosive violence of some transferences of motion. The rate of travel of light when viewed in relation to the intervening distances of the chief bodies of the universe—may appear even slow. More than three years, for instance, are occupied in the transmission of a wave from the *nearest* star to our system.

It may be reasonable then to assume that the possibilities for the existence of a higher rate of intercommunication than this (that of luminous effects) may exist in nature, and that the bodily mass movements of the units of the universe may influence each other more quickly than their molecular movements; since gravitational disturbances or their measures appear to demand this. It is so far certain that in addition to the luminiferous ether there may be plenty of room for finer and therefore more mobile material: or no one, as far as I am aware, has urged a difficulty on this head, provided its presence were subservient to some great mechanical purpose.

Hamburg, May 16. S. TOLVER PRESTON.

Singular Swarms of Flies.

IT may interest some of your readers if I describe a sight which I saw this forenoon, which was quite new to me and apparently to all who witnessed it. After a brisk N.N.E. breeze in the morning, at about 11 a.m. it fell flat calm, the sky becoming inky black, with every sign of a heavy thunderstorm impending. Soon after, looking out of my office window at a belt of trees some hundred yards away, my eye was caught by a most singular and to me (at first) uncanny sight.

Above the trees, apparently one on each principal prominence of their outline, there appeared a number of slim clouds, like straight wreaths of thin smoke, slanting upwards into the sky. Though they maintained their positions, they seemed alive and moving, in a manner partly suggestive of the twisting motion of a waterspout. A field-glass showed the clouds to be swarms of small flies; and looking around, similar swarms were seen above all the trees everywhere. They were perfectly visible to the naked eye a quarter of a mile off, and the glass showed them on the furthest trees in sight, these being nearly a mile away. All seemed to have much the same peculiar slant, pointing more or less towards the (then invisible) sun. Some of the swarms looked to be fifteen or twenty feet long.

Over a few of the low bushes on a bank of rough ground

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close by, similar swarms were to be seen. Even these, however, were inaccessible; but I caught some of an apparently similar swarm drifting over the ground between the bushes, and inclose some of the specimens herewith. To me they look just like the insects which ordinarily strew one's table under the lamp at night (I notice, by the way, that to-night there are none, though the window is open as usual), and therefore I am led to suppose that the special character of the swarms noticed to-day appertains to some condition of the atmosphere, and not to the species of insect; but perhaps some of your contributors can throw light on this point. It would also be very interesting to know whether similar swarms were noticed elsewhere to-day, and whether they showed the same slant as was noticed here.

R. E. Froude.

Admiralty Expt. Works, Haslar, Gosport, May 27.

P.S.—The swarms of flies disappeared about 1 p.m., as the thunder clouds cleared away.

Popular Botany.

We do not expect accurate scientific information from journalists; but so much confusion and error are seldom compressed into a small space as are to be found in a paragraph of which I send you extracts, cut from a London daily:—"A sad case of accidental poisoning by wild hemlock is reported from Tyne Dock. A little band of school children playing on some waste ground had gathered a quantity of a common variety of this dangerous plant, known to country folk as 'fool's parsley.' According to the evidence of one of the party, a little girl aged eight named Pringle, her sister 'said it was cabbage, and she should eat some.' Another boy and girl, named Shafter, who were still younger, followed her example. All three were soon afterwards taken ill. One 'complained of her legs as if they were tired'—a common symptom of hemlock poisoning—and 'her head afterwards got bad.' Pringle ultimately recovered under treatment, but the two Shafters on reaching home gradually became unconscious, and died the same afternoon within twenty minutes of each other. This species of hemlock, known to science as the *Conium maculatum*, is said to be much more poisonous in May than in any other month." It would be interesting to know what the plant really was. It can hardly have been the true hemlock, *Conium maculatum*, and instances of fatal poisoning by fool's parsley, *Aethusa cynapium*, are so rare that an authentic record would be valuable. It is difficult to imagine either of these plants being mistaken for cabbage. Can it have been *Cicuta virosa* or *Oenanthe crocata?* It would be interesting if any reader of Nature could throw light on the subject.

The following delightful paragraph is cut from the same paper a few days later:—

"Can plants see? Darwin gave it as his opinion that some of them can [one would like to know where], and an Indian botanist relates some curious incidents which tend to verify the belief. Observing one morning that the tendrils of a convolvulus on his verandah had decidedly leaned over towards his leg as he lay in an attitude of repose, he tried a series of experiments with a long pole, placing it in such a position that the leaves would have to turn away from the light in order to reach it. In every case he found that the tendrils set themselves visibly towards the pole, and in a few hours had twined themselves closely round it."

Alfred W. Bennett.

Gaseous Diffusion.

In your Notes of last week there is a description of an experiment for showing gaseous diffusion, devised by Prof. v. Dvorák, which, however, does not seem so striking as one that was shown at the Royal Institution more than twenty years ago by, I think, Dr. Odling.

A cylindrical porous battery cell was closed by a cork through which passed a vertical glass tube of about half an inch in diameter. The lower end of the tube was bent upwards into the form of a delivery tube, and was placed in a pneumatic trough, with a cylinder filled with water inverted over the end of the tube. On placing an inverted bell-jar of hydrogen over the porous cell, gas was rapidly collected in the cylinder, and this contained sufficient hydrogen to explode on the application of a flame. On removing the bell-jar, the hydrogen diffused outwards, and water was drawn up the wide tube.

Cooper's Hill, May 29. Herbert McLeod.

NOTES UPON THE HABITS OF SOME LIVING SCORPIONS.

The literature which treats of the habits of living scorpions is not voluminous, but it labours under the disadvantages of being based largely upon undetermined species, and of being often of questionable trustworthiness with regard to the statements that are made. Even accounts that have been given of late years of the same species of scorpion differ widely as to facts of no small importance. Mons. L. Becker, for instance, asserts that the senses of hearing and seeing are highly developed in *Prionurus australis*, the thick-tailed yellow scorpion of Algeria and Egypt; Prof. Lankester, on the contrary, declares exactly the opposite to be the case. Discrepancies such as these and the deficiencies above mentioned show the need for fresh observations upon the subject, and no further excuse need be offered for publishing the following notes upon the habits of some specimens of two species of scorpions, *Parabuthus capensis* and *Euscorpius carpathicus*, which I was fortunate enough to keep for some months in captivity.

For the specimens of Parabuthus I gladly take this opportunity of expressing my thanks to my friend Mr. H. A. Spencer, of Cape Town, who kindly collected them for me at Port Elizabeth, while acting as medical officer on board the Union Steam Ship Company's s.s. *Mexican;* while for the Euscorpius I am indebted to the kindness of Dr. Gestro, of the Natural History Museum at Genoa. This last genus of scorpion Prof. Lankester has also written about; many of my observations, therefore, merely confirm those of this author. No description, however, has to my knowledge ever been published upon the habits of any species of Parabuthus. This genus, however, belongs to the same family as Prionurus, and the behaviour of the two in captivity seems to be very similar.

There is an abundance of evidence that scorpions are nocturnal, and mine were no exception to the rule. They would spend the daytime huddled together in corners of their box or under pieces of wood; at night they would wander about, presumably in search of food. It was easy, however, at any time during the day to rouse them from their sluggishness by applying a little artificial warmth to the box. One end of the box containing the Parabuthus was closed with a plate of perforated zinc. If this box was placed in the fender at a distance of about a couple of feet from a moderate fire, with the zinc end turned towards the grate, the scorpions would climb upon the metal plate and bask in the warmth. But immediately the box was brought near the bars of the grate they would all clamber or tumble from their position with ludicrous haste. It must not be supposed, however, that the amount of heat required to make them retreat was at all great. As a matter of fact warmth that I could without inconvenience bear for several minutes upon my hand would throw these animals at once into a state of the greatest consternation.

When walking both Parabuthus and Euscorpius carry the large pincers or chelæ well in advance of the head; these appendages thus fulfil the office of antennæ or feelers. In Parabuthus the body, however distended and heavy with food, is raised high upon the legs exactly as Prof. Lankester has described in Prionurus, and the tail is usually carried, curled in a vertical plane, over the hinder part of the back. In Euscorpius, on the contrary, as has also been pointed out by Prof. Lankester, the ventral surface of the body is scarcely raised from the ground during progression, and the tail, which is very slender and relatively much lighter than in Prionurus or Parabuthus, is dragged along, extended, and with a slight curl only at its hinder end. This difference in the carriage of the tail depends possibly upon the difference in its size and weight. For it seems reasonable to suppose that the heavy, robust tail of a Parabuthus or Prionurus

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